The cost of the lamps is one dollar each; and, at the present rate of consumption from all causes, the annual expenditure will be 56 lamps. The dynamo is run about 2,190 hours per year (about six hours per day), with an average of about 47½ lamps in circuit, so that the annual lamp-hours would be about 104,025 (2190 × 47.5). Thus it appears that our lamps will, at present consumption, last us in the neighborhood of 1,857 ($\frac{104025}{56}$) hours each.

Description of lamps.

Designation of lamps.	Candle-power.	Resistance in ohms.	Current in ampères.	Electromotive force in volts.
A	32	86	1.180	102
A	16	137	0.745	102
A	10	250	0.400	102
B	8	69	0.745	51
B	16	42	1.200	51

In event of a short circuit through a good conductor, between the wires there would be instantly generated heat of such intensity that the wires would melt, and perhaps the armature also. This heat would in all probability set fire to the wood-work along the line of the wire. To prevent this, Edison has devised his cut-out block, or safety-catch, — a neat device for placing a short piece of alloy in the circuit, which, at 400° F., will melt, and open the cir-

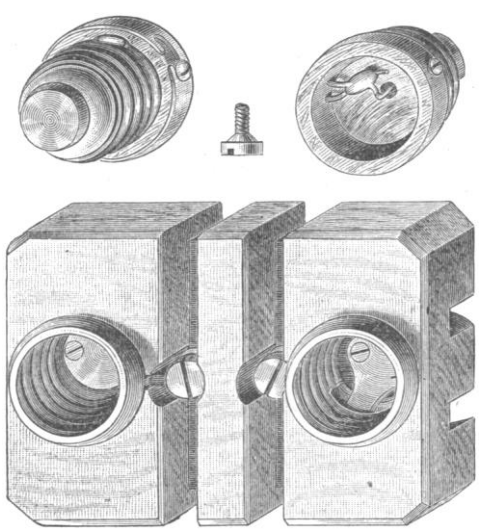

FIG. 15.

cuit. When this happens, all the lamps on the branch circuit fed through that cut-out will be immediately extinguished; and, though one is left in darkness at that point, he is rewarded by a consciousness of greater mischief having been prevented. Fig. 15 represents a double pole cut-out block, a front and back view of the cut-out plug, and a binding-screw. Fig. 16 shows a back view of the same cut-out block, and a section through a cut-out plug. The fusible alloy is contained in the plug, and

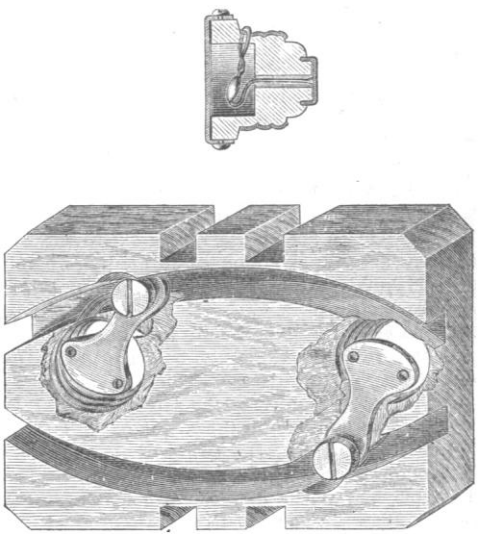

FIG. 16.

is utilized as a solder to unite the two poles of the plug. The plug is made similar to the bottom of a lamp, and the block-socket is similar to a lamp-socket. The wires are held by the binding-screws, and the current passes through the metal of the block and plug. These cut-outs are placed on each of the main circuits, near the dynamo, and on each branch circuit, and always in convenient positions. The alloy in the plug is the only part destroyed by a short circuit, and it is only a minute's work to substitute a new plug.

(To be continued.)

REPORT OF THE GERMAN CHOLERA COMMISSION.[1]

WHEN the commission arrived in Egypt, the cholera epidemic had already begun to decline, so that we could not expect to obtain all the material necessary for carrying out our examinations. Besides, since the termination of an epidemic is the least suitable time for etiological researches, our original plan was to make such preliminary studies as we could in Egypt, and then check our results, as soon as the epidemic had reached Syria, by further investiga-

[1] Report to Minister von Bötticher, secretary of state for the interior. By Dr. KOCH. From the *Kölnische zeitung* of Oct. 1[illegible].

tions in suitable localities in that country. We were able to carry out the first part of our plan in accordance with our wishes; for the commission found abundant opportunity, during its stay in Alexandria, to collect the material necessary for its preliminary researches. This was mainly due to the active cooperation of the physicians of the Greek hospital, who furnished us working-rooms, permitted us to study the cholera cases in the hospital, and placed the bodies of those who died of the disease at our disposal.

At first the commission had two rooms of the hospital, adjoining each other on the ground-floor, and well lighted. In one room the microscope work was carried on, and in the other the culture experiments. The animals experimented on were at first brought into both rooms. But when their number had been increased, it was thought too dangerous to manipulate the material for infection in the same rooms in which we had to spend nearly the entire day, and they were accordingly removed to another room in the old hospital; and there the experiments on infection were made.

The material for experiment was obtained from twelve cholera patients, and ten bodies of individuals who had died of the disease. The cases of nine of the patients were studied in the Greek hospital, two in the German, and one in the Arabian. The symptoms corresponded in all cases with those of true Asiatic cholera. Specimens of the blood, of the vomited matter, and of the dejections of these patients, were taken, and submitted to examination. It was soon apparent that the blood was free from microorganisms, that the vomited matters were comparatively poor in them, but that the dejections contained a considerable quantity; and the latter material was therefore mainly used in the infection experiments with animals.

Although the number of bodies used for dissection was small, the material they afforded was of the greatest service in localizing the disease. They represented the most varied nationalities (three Nubians, two German-Austrians, four Greeks, and one Turk), were of different ages (two were children, two over sixty years of age, and the others between twenty and thirty-five years), and the duration of the disease varied in the different cases. The most important fact, however, is that dissection could be begun immediately or in a few hours after death. The changes produced in the organs, and especially in the intestines, by putrefactive decomposition shortly after death, which render microscopical examination difficult, and its results generally entirely deceptive, were thus effectively guarded against. I wish to lay particular stress on this circumstance, because it is hardly probable that such excellent material for microscopic examination as we obtained could be found in other places.

The appearances of the bodies, as well as the symptoms of the sick, left no room for doubt that we had to deal with true cholera, and not, as at first supposed, with diseases resembling cholera, — the so-called 'choleriform' and 'choleroid' diseases.

We were unable to detect any organized infectious matter in the blood, or in the organs which are usually the seat of micro-parasites in other infectious diseases; viz., the lungs, spleen, kidneys, and liver. Occasionally bacteria were found in the lungs; but it was clear from their forms and position that they had nothing to do with the processes of the disease, but had entered the lungs from the vomited matter by aspiration. In the contents of the intestines, just as in the choleraic dejections, there was an extraordinarily large number of micro-organisms of different kinds, no one of which was present in excessive proportion. There were also no special indications which could enable us to draw any conclusions as to their connection with the disease. On the other hand, the examination of the intestine itself gave a very important result. In all cases but one, a specific form of bacteria was found in the walls of the intestine. The exception was in the case of a patient who had died of a *sequela* several weeks after the cholera had subsided. These bacteria were rod-shaped, and therefore belong to the bacilli. In size and shape they most nearly resemble the bacilli of glanders. In cases where the intestine showed the slightest evidence of change, the bacilli had penetrated into the tubular glands of the mucous coat, and had set up a considerable irritation there, as was shown by the distension of the glands, and the accumulation of many-nucleated round cells in their interior. In many cases, too, the bacilli had worked their way beneath the epithelium, and had penetrated between it and the gland membrane. Moreover, they had planted themselves in large quantities over the surface of the villous coat of the intestine, and had penetrated its tissue. In severe cases of the disease, where there was a bloody infiltration of the mucous coat of the intestine, the bacilli were present in great numbers, and had not confined themselves to the invasion of the tubular glands, but had entered the surrounding tissue in the deeper layers of the mucous coat, and sometimes had penetrated to the muscular coat. The villous coat was also thickly covered with them in such cases. The principal seat of all these changes was found to be the lower part of the small intestine. In investigations like the present, the examination ought to be made on perfectly fresh bodies, because putrefactive decomposition can induce similar growths of bacteria in the intestine. From this consideration I was unable last year to attach any value to the discovery of the same bacilli in the same position in the intestines of cholera patients which I had obtained directly from India, because I was afraid of complications with *post-mortem* changes. This former discovery, which was made in the intestines from four bodies of persons who had died of cholera in India, is now confirmed, because there can be no error in the present case due to decomposition in the Egyptian bodies. The inference, too, that the correspondence in the conditions of the intestine in the Indian and Egyptian cholera may be taken as a further indication of the identity of the diseases, is not without weight.

The number of bodies of cholera patients used by the commission for examination, it is true, was small.

Bacilli were found in all the fresh cholera cases, but were wanting in the case where death had occurred after the symptoms of cholera had disappeared, and in others where death had not occurred from cholera, in which examinations were made for the sake of comparison; so that there can be no doubt that these organisms have some connection with that disease. But it is too much to conclude yet that bacilli are the cause of cholera because they are found in the mucous coat of the intestine of cholera patients. The inference might be reversed; and we might say that the disease produces such changes in the mucous coat, that some of the many bacteria present there as parasites are able to penetrate the tissue. A decision of the question, which of the two views is correct, — whether the infection or the bacteria invasion comes first, — can only be settled experimentally by collecting the bacteria from the diseased tissue, breeding them by 'pure culture,' and then reproducing the disease by infection experiments on animals. For this purpose it is necessary, first of all, to have such animals at our disposal as are susceptible to the infectious matter; but in spite of every effort to infect animals with cholera, it has not yet been demonstrated that they can be made to take that disease. Experiments have been tried with rabbits, guinea-pigs, dogs, cats, monkeys, pigs, rats, etc., but always without success. The only statements to the contrary, worthy of notice in this connection, are the accounts of Thiersch's experiment with mice which he fed with the contents of the intestine of a cholera patient, and which were then attacked with diarrhoea, and died. This experiment has been confirmed by reliable experimenters, like Burdon-Sanderson, and has been criticised, and the conclusions drawn from it disputed by others. We considered it necessary to repeat this experiment because it was of the utmost importance for our purpose to discover some species of animal capable of infection with cholera.

Fearing that the requisite number of mice could not be obtained at once in Alexandria, we carried fifty of those animals with us from Berlin, and began the infection experiments with them. We also experimented with monkeys, — because they are the only animals susceptible to certain human diseases, such as small-pox and relapsing fever, — and, besides these, with a few dogs and chickens. But, in spite of all our care, the experiments were failures. The most varied samples of vomited matter, choleraic dejections, and contents of the intestines from bodies of persons dead of cholera, — sometimes fresh, sometimes after standing some time in a cool or warm place, and sometimes in a dried condition, — were fed to these animals; but no symptoms of cholera appeared, and they remained perfectly healthy.

Besides this, 'pure culture' experiments were made with bacilli taken from the contents of the intestine and its walls. The material obtained in this way was fed to the animals, and inoculation was also tried. This latter method, with the products of the 'pure culture,' sometimes produced septic diseases; but no symptoms of cholera appeared.

That the material of the disease is very often contained in the dejections of cholera patients in an active form, is shown in several ways, and particularly by the frequency of the disease among washerwomen who wash clothes soiled with such dejections. A case in point occurred during the present epidemic in the Greek hospital, where a washerwoman who washed for the cholera hospital exclusively was taken with the disease. It is therefore certain, that, of the numerous samples used in our experiments, some at least must have contained the infectious matter. But since our experiments were failures, it must be assumed, either that the animals we used are not susceptible to cholera, or that we had not discovered the right method of producing infection. At any rate, the experiments ought to be repeated and modified; but, with the material now at our command, there is little prospect that they would prove successful.

Our want of success may possibly be explained in still another way, which is this. In a place visited by the cholera it is usual for the disease to cease before all the inhabitants have been attacked; and, although the infectious material is scattered about in great quantity, fewer and fewer persons are affected by it, and the epidemic at last dies out in the midst of individuals capable of taking it. This circumstance can only be explained on the theory that the infectious matter loses its activity, or at least becomes uncertain in its action, towards the end of the epidemic. If, therefore, human beings become less susceptible to the infection of cholera towards the end of the epidemic than at its outbreak, it can hardly be assumed that the animals used for experiment, of whose susceptibility to infection we know nothing, should differ from them in this respect. And for our experiments we could only obtain material which had been collected towards the end of the epidemic, and which must be presumed to have been more or less inactive. It is of course possible, that under suitable conditions, such as the outbreak of a cholera epidemic, the infection of animals with the disease might be successfully accomplished; and such would be the proper time to determine by experiment whether the bacilli I observed in the mucous coat of the intestine constitute the true cause of cholera.

However far the commission may still be from a complete solution of the problems proposed to it, and although its labors have contributed little which may prove of practical value in combating the cholera, yet, considering the unsatisfactory conditions under which the experiments were made, and the short time the commission was able to devote to them, the results thus far obtained should be regarded as encouraging. The experiments fully answer the original purpose of localizing the disease, and, by ascertaining the constant presence of characteristic micro-organisms, have supplied the first condition for the investigation of infectious diseases, and afforded a determinate object for further research.

From the foregoing statements, it is clear that the commission can accomplish no more in Alexandria

than has already been done. It might be thought that it could pursue its investigations in some other place in Egypt where the cholera prevails, but there are insuperable objections to such a plan. The cholera has disappeared from all the large cities of the country, and only holds its own in the villages of upper Egypt; and an attempt to carry on our experiments in that part of the country would meet with the strong disapproval of the Egyptian government on account of the disagreeable complications in which the condition of affairs there might involve us. Moreover, I have been assured by responsible persons well acquainted with the Egyptians, that it would be impossible to obtain material for dissection in Egyptian villages; and for these reasons I must renounce all hope of following the course of the cholera up the Nile. The disease also, contrary to all expectation, appears to have gained no foothold in Syria. Since the investigations now in progress will occupy only about two weeks, the work will soon have to be temporarily suspended. The commission, however, entertains a strong desire to prosecute its researches further, and satisfy the object for which it was created. It would be a great disappointment if the results it has already reached should prove fruitless from want of further experiments. The only opportunity which is afforded us at present for continuing our researches is in India, where the cholera is still prevalent in several large cities, particularly in Bombay, and is not expected to subside immediately. It is also probable that we could gain access to some hospital there, and repeat the work which proved so valuable in Alexandria. In case, in your excellency's opinion, it should be deemed advisable to continue the researches of the commission, and extend the field of its labors to India, I am ready to continue in charge of its management.

I must also say a few words about the additional labors which the commission has found time to prosecute in connection with its researches on cholera. Egypt is full of parasitic and contagious diseases, and it was not difficult to obtain suitable material for the examinations we wished to make in order to control the results obtained in studying the cholera, and also to settle some general questions bearing on infectious diseases. For example: I dissected the bodies of two persons who had died of dysentery. In one case, where the patient had died of an acute attack, there were parasites in the mucous coat of the intestine which did not belong to the bacteria group, and were unknown. I also dissected the body of an Arab who had died in the Arabian hospital of malignant disease. The disease in this case was probably taken from sheep, which are imported into Egypt from Syria in great numbers, and die of anthrax *en masse*. I was also afforded an opportunity to observe six cases of bilious typhus in the Greek hospital. This disease closely resembles yellow-fever, with which it is often confounded, and presents much interest to the student. Three of these patients died, and were dissected.

Besides this work, repeated examinations were made of the micro-organisms in the air, and the drinking-water of Alexandria. If time allows, I intend to study the Egyptian ophthalmia.

The labors of the commission, which from their nature were very trying and fatiguing, and for the most part of a disagreeable character, were rendered doubly irksome by the high temperature prevailing in the city. It has been impossible to interrupt the work a single day until now. Nevertheless, the members of the commission are in good health, and have only suffered from some slight complaints, due to a change of climate, which soon disappeared. However, as soon as the condition of the work will allow, I consider it advisable for the commission to rest a few days. I intend, therefore, to go with it to Cairo for a short time, partly for the sake of recreation, and partly in order to visit the principal seat of the cholera in Egypt, and make further observations there.

THE PHYSIOLOGICAL STATION OF PARIS.[1] — I.

WE have seen in the last few years all kinds of establishments erected to provide for the new needs of science. Laboratories, although great discoveries have been made in them, have become in certain respects insufficient. In the study of organized bodies, as in that of the physical forces of the earth, one is soon brought to a standstill if he cannot study nature in her own domain.

Special establishments for certain sciences, astronomy for instance, are a necessity; and lately naturalists have perceived the insufficiency of the means placed at their disposal. Maritime stations, gardens for acclimation, experiment stations, agricultural stations, stations for vegetable chemistry or experimental medicine, — all these have responded to the development of certain branches of science.

Physiology, almost the only exception, has been, up to the present time, dependent upon laboratories. These are, in France at least, wretched places, poor and unhealthy, where the investigators are obliged to live in the hope of discovering the properties of the tissues, and the functions of the living organs. There is discovered the action of medicines upon the living organism, of poisons, and the various chemical and physical agents; there, by means of vivisection, or by the use of the proper and delicate instruments, the inner mechanism of the vital functions is analyzed.

This condition of destitution could not continue. It is evident, that with the means at its disposal, within narrow limits, and compelled to operate upon a few lower animals, physiology could not but remain behind the other sciences. In any case, it could not hope to attain its full development: it must abandon, without practical application, the knowledge that it had obtained at the cost of so great efforts.

In the last half-century physiologists have written a large number of works on the nervous and muscular systems. We have learned to distinguish the nerves

[1] By E. J. MAREY of the French institute. Translated from *La Nature*.